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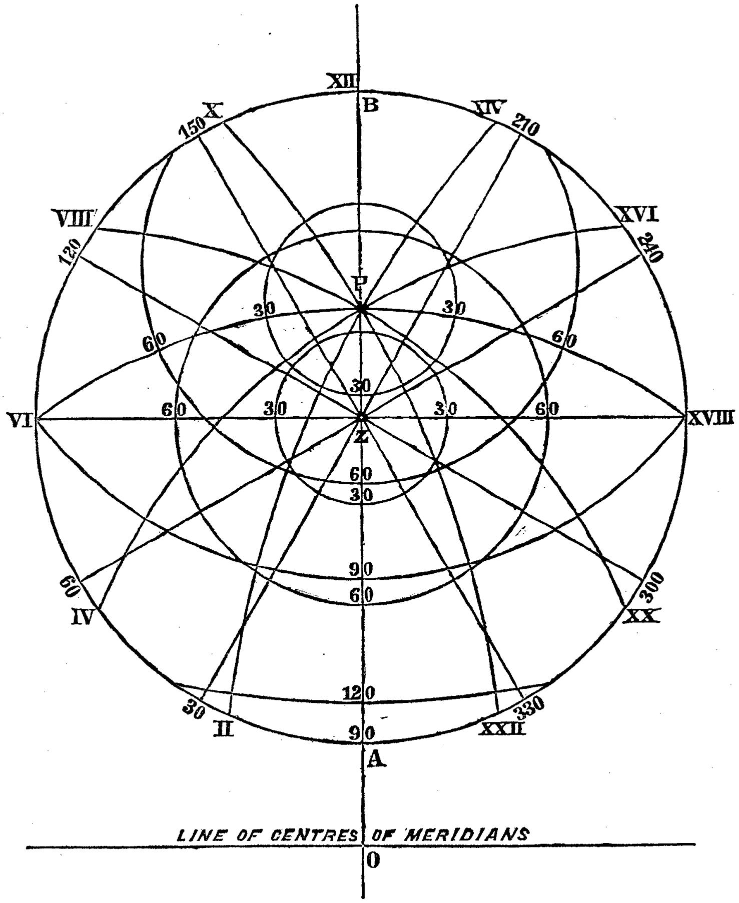
muth, and tabulate them for reference. The table would then supersede the diagram.

Stereographic projection for the conversion of polar-distance and hour-angle into zenith-distance and azimuth.



(Fig. 3). Lat. $52\frac{1}{2}^\circ$.

If it were considered advisable, diagrams such as the present could easily be constructed, for use with the diagonal altazimuths at each of the five English stations for the observation of the transit of *Venus* in 1874.

An abridgment of the diagram for latitude $52\frac{1}{2}^\circ$ is shown in fig. 3.

St. John's College, Cambridge,
October, 1872.

On the Examination of the Photographs taken during the Total Solar Eclipse of December 11–12, 1871, *at Dodabetta.* By Lieut.-Col. J. F. Tennant, R.E., F.R.S.

In the text of my report on the eclipse, I have expressed my opinion that the relation between some of the prominences and

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neighbouring features of the corona is so marked that it points to an evident connexion. A careful examination quite confirms this; and I believe any one who has access to the photographs will be led to the conclusion that the relative positions of the traces of the prominences and of the coronal features are unchanged, and that the Moon, which we know to move with respect to the former, does so equally with respect to the latter. It has, however, been thought most desirable that this general impression should be confirmed, and I have now to describe the measures which have been taken to this end; in doing so I would, however, wish to say, that Dr. De La Rue has aided me every way with apparatus, advice, and time; and that, indeed, without him the data for this memorandum would not have existed.

In the first place, and as a preliminary to other steps, the negative photographs were placed in an oxy-hydrogen microscope, and projected on to mounted drawing boards, on which the main features were sketched. The enlargement of the Moon's diameter (about 24·5 times) gave it about 7·64 inches, and care was taken to keep this constant by retaining the adjustments. Careful tracings were made of the sketches, and an attempt was made to determine roughly the amount of the lunar motion with respect to the corona. Two sketches had been made of the projection of No. 1 photograph; one, mainly by Dr. De La Rue, is spoken of as No. 1, while the other, mainly by myself, is distinguished as No. 1 T. In the following process the results from these furnish some check on each other:—

A tracing, say of No. 2, was laid on sketch No. 1, and the greatest coincidence of the general features of the corona produced by shifting the tracing on the sketch, each tracing and sketch having had marked on it a circle which generally coincided with the lunar limb shown on it, and the distance of the centres of the circles was assumed as the apparent motion of the Moon with respect to the corona, as deduced from those two pictures. In the first place, it was found that the lunar motion so deduced was always in the right direction, or towards the following side, and slightly to the south. To come to details, comparing

No. 1 with	No. 2	the displacement was	0·055 inch.
,,	No. 3	,,	0·110
,,	No. 4	,,	0·180
,,	No. 5	,,	0·195

In dealing with these numbers it became necessary to assume an epoch for each picture. Dr. De La Rue thinks that the instant of opening the lens should be the assumed epoch, but I propose to use the middle of the exposure, which seems to me more satisfactory; and I have ascertained that the difference will not be important. From the moments of exposure and closing the lens, given on page 11 of my report, I find the most probable value of

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the motion of the Moon in one minute to be 0·150 in. on the scale of the tracings.

Again, comparing No. 1 T in the same way with each of the same tracings, we have

No. 1 T compared with	No. 2	gives for the motion	0·110	inch.
„	No. 3	„	0·125	
„	No. 4	„	0·210	
„	No. 5	„	0·250	

It is quite evident that there is a considerable difference shown by these figures in the estimated centres of No. 1 and No. 1 T; considerable, that is, with respect to the quantity to be determined. The mean amount seems to be 0·035 in., and if this quantity be deducted from all the motions formed by comparisons with No. 1 T, we have a probable motion from these comparisons of 0·1485 in. per minute. The motion which I have derived from computation is 0·107 in., and it will be evident that the error is not more than might be anticipated from the process; while, at the same time, it appears clear that there is an actual motion of the Moon with respect to the general corona, just such as exists with respect to the prominences and Sun.

In the course of these comparisons it became evident that the resemblance between the photographs was no general one, but that every ray, or flame, and rift, could be traced through the whole of the photographs employed in them, though considerably changed sometimes by the varying circumstances of the photograph. This circumstance rendered more probable success in measuring the details of the corona with Dr. De La Rue's micrometer, which had been proposed.

Accordingly a catalogue of objects was made, and No. 1 Photograph having been placed on the micrometer-table, and duly centred, 26 position-angles of rays, and salient angles, were measured with corresponding distances, and ten angles and distances of the bottoms of rifts. Where there was an appearance of two or more envelopes, separate observations were made, but the set is counted as one. Photograph No. 3 was then chosen for comparison, and placed on the table, and measurements were taken of the corresponding points, which were without difficulty identified. One remarkable difference was noted. In No. 1 there is a remarkably long ray, with a position angle of 99°, which projects faintly as much as 46′ from the lunar limb. In No. 3 this was much restricted, and very difficult to distinguish from a neighbour which, in No. 1, was not two-thirds of the length. This, however, must not be considered evidence of change, as every ray in No. 3 is less extensive than in No. 1, and this long faint ray may well have suffered more than its more marked neighbour, from the cause which has shortened all the rays.

The greater part of these observations was made by Dr.

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De La Rue, and verified by myself. A commencement was made by the reverse procedure, but the greater delicacy of Dr. De La Rue's eye soon became evident, and he very kindly gave the time necessary. Under these circumstances, I am, perhaps, hardly able to judge of the value of the measurements; but as far as it is worth anything, my opinion is, that while entirely confirming the statement of the general permanence of the corona, these measurements do no more. The objects to be observed are very ill defined, and towards the limits of the silver deposit forming the photograph it is sometimes impossible to see even traces of it, at the same time as the wires of the micrometer. A reference to the neighbouring specks on the film is, in these cases, inevitable, and it must be difficult in the extreme to avoid noting mentally the position of the estimated points of a ray as compared with these definite marks, and once this is done I cannot feel that accordances give me any evidence of value. I propose therefore resting the evidence of lunar motion on the evidence of the previous examination, and omit the figures representing the positions determined.

Before closing, I may mention a further experiment. One edge of a rift, that which I have spoken of in page 7 of my report as nearly vertical, and which was the first across which I laid the slit, was so well marked that it seemed practicable to determine if this edge moved with respect to the Moon's centre, and its position made the direction of the lunar motion not far from perpendicular to the line. I, therefore, determined the co-ordinates of a point on the estimated boundary with reference to axes passing through the lunar centre, one of which was parallel and the other perpendicular to the direction of motion. If there were no change in the corona the latter ordinate would alone be altered, and in examining No. 3, the ordinate in a direction parallel to the lunar motion was measured for the same value of the perpendicular ordinate. This showed an apparent motion of 0·046 in., (the lunar diameter being 0·312 in.), an enormous amount plainly ascribable to reduced effect of the light in the second picture; and it shows how unsafe a comparison, involving an edge, is. But when the wires were placed in the same position relatively to the Moon's centre, as in No. 1, it became quite evident that no increased action of the light could have placed the side of the rift in such a position, and that there was real motion.

On the whole, I think there can be no question that there is a real motion of the Moon across the generally constant phenomenon of the corona, and this being admitted, I anticipate that the conclusion will be generally accepted, that the portions of the corona shown on the Dodabetta photographs are solar. Such being, I believe, the deduction from the evidence they alone supply, the fact that they very closely resemble the pictures taken by Lord Lindsay's assistant at Bekul, strengthens the conclusion, and shows that the changes are not sensible even in longer intervals than those between my extreme pictures.

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Probably many will think I might have relied more on our measurements, but I shall be content if they accept my conclusions; and I trust that the Royal Astronomical Society will be able to give, with my papers, such engravings of four of the photographs as will show the kind of agreement which exists.

Deal, August 24, 1872.

On the Arc of the Meridian Measured in South Africa.

By I. Todhunter, M.A., F.R.S.

1. It is well known that in 1752 La Caille measured an arc of the meridian in South Africa, and that the result obtained was decidedly larger than might have been expected from the measurements in the Northern hemisphere. It is also well known that Sir Thomas Maclear, formerly Astronomer Royal at the Cape of Good Hope, has in recent times carried on another measurement in South Africa, far more extensive than La Caille's, and that the result of the new operation is in harmony with the average of the trustworthy Northern arcs. Various references to these recent operations will be found in the *Memoirs* and *Monthly Notices* of the Astronomical Society; but I think no account has been given in these publications of the two large volumes issued in 1866 containing the details of the astronomical and geodetical work. There can be but one opinion of the value of Sir Thomas Maclear's measurement, and of the zeal and energy with which it was conducted. The Royal Society has marked its approbation by conferring a medal on the eminent astronomer; see the *Proceedings of the Royal Society*, vol. xviii. p. 109. The Lalande medal of the Academy of Sciences at Paris has also been awarded to Sir Thomas Maclear for this work.

2. The title of the volumes to which I have alluded is *Verification and Extension of La Caille's Arc of Meridian at the Cape of Good Hope*, by Sir Thomas Maclear, Astronomer Royal at the Cape of Good Hope. These volumes were published in England, under the editorship of the Astronomer Royal.

3. In consulting these volumes for the purpose of a *History of the Theories of Attraction and the Figure of the Earth*, it has appeared to me that there are some points in which explanations and additions are required; and I hope it may not be impossible to have a supplement of a few pages, for the sake of giving completeness to the publication. There must be at present various persons who are qualified to throw light on the subject; and thus it may happen that information could now be supplied which in a few years will be quite unattainable.

4. As the volumes now stand there is a very great discrepancy between the title and the contents. Instead of having "a verification and extension of La Caille's arc," we have the account of a new arc; and there is no comparison whatever of La Caille's *length* with the new *length*, though there is a comparison of La Caille's